REVIEWS.

GENERAL ANÆSTHESIA AND ANÆSTHETICS.

THE PRACTICE OF ARTIFICIAL ANÆSTHETICS. By DUDLEY W. BUXTON, M.D., B.S., M.R.C.P., Administrator of Anæsthetics at University College Hospital.

ON ANÆSTHESIA. By R. MILNE MURRAY, M.B.

ON ANÆSTHETICS. By JOHN CHIENE, F.R.C.S. Ed., F.R.S.E., Professor of Surgery in the University of Edinburgh.

THESE are the titles of a series of papers read before the British Medical Association at its last meeting, and published in the *British Medical Journal* for Sept. 19, 1885. Such a series of papers upon anæsthetics written by men selected, as especially fitted for the purpose, by a society so high in standing and so fruitful in good works as the British Medical Association, cannot but attract the attention and excite the interest of every one. Such papers deserve careful examination. From them may be learned what, at the present time, can be accepted as settled in regard to anæsthetics; what, if any, recent advances have been made; what points yet remain upon which opinion is divided. From them our readers will be enabled to compare the theory and the practice of surgical anæsthetics here, with those of their professional brethren of another country, of one which has done so much, in so many ways, to advance the knowledge and promote the use of anæsthetic agents.

These papers are not, of course, elaborate or exhaustive, no one of them extending beyond a length for comfortable reading at a single sitting. The one by Dr. Buxton is largely, and that by Dr. Murray entirely, devoted to the physiological aspects of the subject, while that by Mr. Chiene, and the debate, which is brief, tend largely toward the more practical points. All of them bear evidence of thorough acquaintance with the subject, all show care and study in the preparation and an evident effort to bring the subject up to the latest time. One notable exception, however, must be made to the last clause of this general statement. The paper by Mr. Chiene is stated to be a reprint of a lecture delivered by him to his class in 1876. Now whether it is just the proper thing to present to such an Association, upon a subject so important as this, matter ten years old, is for others to determine. This much, however, is very certain: if the profession has not advanced as to anæsthetics within the last decade beyond Mr. Chiene's paper, or farther than is indicated in the few added passages placed in brackets, then there is much to deplore. Further, it may be said, that the man who can say that he has "nothing to retract and very little to add" as to his knowledge of the subject as it was ten years ago, is strangely oblivious to what has gone on around him.

The first conclusion arrived at from perusal of these papers is, that there are some points concerning anæsthetics which seem to be settled. These may be first disposed of. Thus, we find no mention of "idiosyncrasy" as an explanation of death under these agents. It has figured all through the history of the subject, but with gradually decreasing importance. It was a bugbear which should have been banished after even the first few deaths had occurred to persons who had taken the anæsthetic before, because idiosyncrasies do not change from day to day. It seems now to have disappeared; let us hope that it be forever.

Etherization by the rectum finds here no assistance toward professional favor. It receives but scant mention, no praise, and full recognition of its dangers.

The conclusion seems to be, that the agents which are now in daily use are the best known. No new anæsthetic is brought forward and the use of none but the standard ones advocated. Methylene is again stated to be nothing more than diluted chloroform. Methylene dichloride is "a substance very difficult to prepare and very costly," while amylene and ethidene have a bad record as to safety. Effort, is therefore, to be directed to a better knowledge of the action of those we now have, rather than to "a superficial, if showy, acquaintance with the plethora of agents which already glut the anæsthetist's armamentarium." No one who reflects upon the immense experience we have had with ether and choloroform, can fail to assent to this. A vast length of time must elapse before we could possibly acquire an equal amount of experience with other agents, and when we consider the low rate of mortality from the agents we now have, we can see how much longer would be the time before we could be sure that others are safer. Possibly chemistry may make some brilliant discovery of a general anæsthetic which, like cocaine, will abolish pain without affecting consciousness, but the probabilities are, that the anæsthetics now in use will not be displaced.

Discussion seems to have ceased as to the mode of action of anæsthetics. Dr. Buxton alone takes up the subject at length, and considers the theories that they produce their effect: 1, by lessening oxidation of the tissues; 2, by causing changes in the blood akin to carbonic acid poisoning; 3, by causing anæmia of the brain. None of these is tenable, and he falls back upon the only possible explanation—some change in the minute structure or processes of the nervous system.

> "We, therefore, have to fall back upon one hypothesis which formulates that the action of this group [the carbon compounds] of anæsthetics is one exerted upon the nervous system itself. To this view a rider will have to be appended, to the effect that this action is one which falls upon the protoplasm of the nervous and of the muscular systems."

All other theories of the action of anæsthetics were disposed of, and this same conclusion arrived at, by Claude Bernard,[1] more than ten years ago. Although this author is quoted by Dr. Buxton, nothing is said as to his explanation of the nature of the action, which was that it "is a semi-coagulation of the intimate constituents of the nervous cells," a theory which does not break down before the fact that the movements of the sensitive plant, where no nervous system exists, can be arrested by chloroform. Leave out the word "nervous," and the application holds good. However, knowledge as to the particular change, whatever it may be, is of no practical importance.

---

[1] Leçons sur les Anæsthesiques, etc., Paris, 1875.

We find in these papers a general expression of the opinion that all anæsthetics are dangerous. There is no dissenting voice. Dr. Buxton says:

> "In summing up the case of various members of the carbon series in which chlorine occurs, I think we must admit that, as far as the evidence is now before us, they are all dangerous anæsthetics."

Mr. Chiene says: "I am of opinion that no form of anæsthesia is safe, and to aim at obtaining an absolutely safe anæsthetic is to follow a shadow." Mr. Gunn, in the debate, says: "A perfectly safe anæsthetic had not been found, nor was it likely ever to be obtained." This may seem a small matter to comment upon, and so it is when the physiology of profound anæsthesia is alone considered, but it is far from being so in view of the partisanship which has prevailed, and which is still sometimes indulged in, as to particular anæsthetics. There is no agent used which merits the distinction of being the *only* safe anæsthetic. Let those who have claimed this for ether make a note. Moreover, there is a practical bearing of the fact well put by Mr. Chiene: "There is danger in lauding any anæsthetic as safe; it will certainly make the average administrator careless."

The next and very important observation is that the great danger of partial anæsthesia seems at last to have received due recognition. It is admitted by nearly all, and it is stated by one, who is not far from the truth, to be "the worst thing possible." Clearness of understanding of the mode of death, when it follows the surgeon's first incision under partial anæsthesia, is not promoted by saying that the death is by "shock," or by "syncope." Nevertheless, an appreciation of the great clinical fact and its practical value, is of the first importance. Looking back over the history of anæsthesia, it is astonishing to see how slow the profession has been in giving general acceptance to so important a point. Worked out experimentally by Lallemand and Perrin,[1] recognized and impressed by Richardson,[2] presented to and urged upon the profession of this country by the writer as early as 1867,[3] it has never until now assumed the prominence it deserves. Yet clinical experience has, unfortunately, given but too many illustrative examples. Over and over again death has followed instantly upon extraction of a tooth. Forty per cent. of deaths have taken place when the anæsthetic was given for a minor operation. A large proportion of these find their explanation only in this way. Now that this reflex mode of death is recognized in standard works on materia medica,[4] there will be no further question as to it. Dr. Brunton, however, explains the fact that imperfect anæsthesia is so dangerous, by saying that "chloroform does not paralyze all the reflexes at the same time." Claude Bernard says that in the early stages of chloroform administration the reflex actions are *increased*. We do not consider it yet established that under ether this mode of death is equally likely to occur. Still, caution is the best safeguard. The ultimate action of all anæsthetics is probably the same. In the language of Bernard, there are many anæsthetics, but only one anæsthesia.

---

[1] Traité d'Anæsthesie Chirurgicale, Paris, 1863
[2] Med. Times and Gaz., 1870.
[3] This Journal, January and October of that year.
[4] Brunton's Pharmacology, Phila., 1885.

We unhesitatingly accord to Dr. Murray the credit of furnishing the most considerable and the best addition to our knowledge of the subject of anæsthesia to be found in these papers. Engaged in a series of experiments on the action of the uterus in rabbits, some modifications of respiration under chloroform forced themselves upon his attention. He observed that the movements of respiration may continue normal as to rhythm and depth, and yet no air enter or leave the chest. The chloroform was administered through a tube introduced into the trachea and the air-current registered by a recorder. The failure of the recorder to move was observed in five different cases, and the cessation extended in one case to twenty-eight seconds. All points of deception having been investigated, the conclusion given above was forced. Dr. Murray's explanation of this occurrence is that it arises from a failure of coördination of the muscles of respiration; that their rhythmic action is disturbed, as is the faculty of speech by the influence of alcohol, producing what may be termed a "respiratory stammer."

> "Thus it might come about through the action of the anæsthetic on the centre of respiration, that the action of the diaphragm might be antagonized by the muscles of respiration, and *vice versâ*."

Dr. Murray further observed that when the recorder ceased to register an air-current, if a sudden afferent impulse were given, as by pinching the eyelids, it immediately recommenced work, showing that normal respiration was resumed. This brings us to the practical application which he makes. Sudden cessation of respiration in the human subject is generally explained by supposing that the tongue falls back on the glottis, obstructing the passage, and by pulling the tongue forward the patient is saved. This is doubted. The position is taken, with diffidence, that in many of these cases the falling backward of the tongue had nothing to do with the cessation of respiration, which, instead of depending on local causes was central, instead of being mechanical was vital. The relief which follows pulling forward the tongue depends then, in some cases at least, upon a reflex influence exerted upon the respiratory centre, similar to that which pinching the eyelids effects, and not upon mechanically opening the glottis. Now it is curious to note that Dr. Murray does not mention any authority in connection with this theory of restoration, he seems to have reached it directly from observation of his own experiments. But Lister[1] taught this doctrine fully and clearly; he both called in question the falling back of the tongue, and maintained the reflex influence exerted by pulling it forward. Mr. Chiene, in his paper, agrees with Dr. Murray. He says that he is inclined now to lay greater stress upon the reflex influence of pulling out the tongue than he formerly did, although he does not entirely abandon the mechanical theory. Finally, the theory of this reflex influence finds strong confirmatory support from the progressive march of chloroform anæsthesia. The mucous membrane of the throat and larynx is the last refuge of departing sensibility.[2] Based upon this fact, even before 1863, Jules Guèrin advocated cauterization of the pharynx as a means of combating the accidents of anæsthesia.

---

[1] Holmes's System of Surgery, art. Chloroform.

[2] Lallemand et Perrin, op. cit.

All that Dr. Murray has written here upon the resuscitation of animals when respiration has ceased, should be read. It is highly interesting, and there is a further practical application. He is undoubtedly right in the assumption that the effect of the first inspirations made is to carry more chloroform into the blood and thus seal the fate of the patient; and that, therefore, the first effort should be to withdraw from the air-passages the air saturated with vapor. While it is entirely impracticable to follow out his suggestion, to carry a tube, which, in case of accident, is to be passed into the glottis, we think his teaching worthy of being kept constantly in mind by every administrator. He should satisfy himself that air passes in and out of the chest, as well as that the respiratory muscles move, and that in efforts to restore the patient when in danger, expiration should precede inspiration. We may add, too, that the administrator of chloroform should never fail to hold in mind the residual air of the respiratory process.

The mode of death from anæsthetics equals, if it does not transcend, in interest and importance, any other point connected with the subject. We are glad to find here a general recognition of the fact, that there is more than one way by which these agents prove fatal. No one thing, in our opinion, has retarded progress so much as want of recognition of this fact. There have been two great errors in the past as to this point: first, an obstinate adhesion to some particular mode of death; second, a too rigid application of the results produced in animals to man. They have not yet entirely disappeared, for there are traces of them even here. Thus, Dr. Murray says "chloroform is primarily a respiratory poison, and if given in sufficient quantity it stops the action of the respiratory centre before it affects the heart." The proposition is true if limited to animals, but it can be broken down in a moment by clinical evidence. If this were matter of theory alone it would not merit attention, but theories in regard to the mode of death have direct practical bearings of the utmost consequence. From theories have been derived those sententious expressions which have influenced practice, as do all dogmatic expressions, and influenced it for evil. "Give a dose of spirits before administration and your patient is safe." "Never mind the pulse—watch only the respiration," etc. The long continuance of error upon this point can only be explained by the facts, that it has been given to no one man to see and compare many deaths, and that there have never been spread before the profession the collected deaths so that they could be studied and compared. In this country the deaths were classified as early as 1867,[1] and an attempt made to impress upon the profession the importance of recognition of the fact that they differ widely in character. Nothing can be more striking to any one who will carefully study anæsthetic deaths, than the different period of the process at which, and the different manner in which, the subjects died.

Dr. Buxton's paper is the fullest upon this point. He recognizes reflex death by chloroform of two forms. The first is when the impression comes from the surgeon's knife in a state of partial anæsthesia, which has been already considered. In the second the impression is upon the terminal branches of the vagi in the lungs, or upon the laryngeal nerves, reflected upon the heart and suddenly inhibiting its action. This doctrine was first taught by Dr. Bigelow, of Boston, as early as

---

[1] This Journal, October No.

1848,[1] then by Brown-Séquard in his lectures on the physiology of the central nervous system, and has been repeatedly urged upon the profession in this country since. It is sustained by the experimental observations of Rutherford,[2] upon the effect of irritating vapor, as ammonia, applied to the nose of anæsthetized animals, and especially by the observations of the Anæsthetic Committee of the British Association[3] as to the sudden and very considerable diminution of the blood-pressure upon every application of a fresh supply of chloroform to the sponge. Recognition of this mode of death is of the first practical importance as to administration. Case after case in the clinical records can be pointed out, in which death followed instantly upon pouring out a new supply of the liquid, or upon the patient drawing a deep inspiration.

As there are two forms of reflex death, so there are two forms of direct death—*i. e.*, death produced by direct action of the agent on the nervous centre of either the respiratory or the circulatory systems. Illustrations of both these can be found in any collection of deaths; sometimes respiration has ceased first, sometimes cardiac action stopped suddenly while the breathing continued for a time. Both these forms are recognized by Dr. Murray.

Besides these modes of death, together with that from obstructed respiration, either by spasm of the glottis, falling back of the tongue, or incoördination of the respiratory muscles, as already considered, Dr. Buxton is inclined to accept another, caused by direct action of chloroform upon the muscular structure of the heart. The question of this mode of death was considered by Koch,[4] by Kappeler,[5] and by Richardson. All have decided in favor of the view that the influence is exerted rather upon the cardiac ganglia than upon the muscular structure. It would seem impossible to say what particular tissue is affected when chloroform is blown on the heart of an animal and its action thereby restrained or stopped. All analogy, together with the marked preference, or elective affinity, of chloroform for the nervous system, would seem to sustain the opinion of these authorities. Dr. Buxton, however, thinks the effect is exerted upon the protoplasm:

> "Chloroform is certainly inimical to protoplasm at the least in a higher degree than is ether." "There seems to me no doubt at all that chloroform is a powerful protoplasm poison, and as such its use must be fraught with danger."

As to modes of death from ether, Dr. Buxton recognizes, first, that from spasm of the glottis, "so rare as to be practically unimportant, save as a means of pointing a moral." Second, that by causing bronchitis, pneumonia, etc. "That there is a considerable danger of these occurrences in the case of young and delicate children, I am pretty certain." It is the aged, we believe, who are peculiarly liable to a rapidly fatal suffocative catarrh after ether, although full references cannot now be given.[6] Mr. Chiene mentions a death the day after the operation from severe bronchial symptoms, and in his opinion due to the ether, but he does not give the age of the patient. In the debate one speaker recognizes this danger. Then comes death by paralysis of the respiratory centre, and claimed by those who carry their advocacy of ether to partisanship, to be the only mode of death from this agent. Dr. Buxton admits another, the same mode as that in which chloroform frequently proves fatal, a death by paralysis of the circulatory centre, for so we interpret the term "syncope." This form, he says, is rare, much rarer than with chloroform, which is certainly true. But it is very singular to see that Dr. Buxton having admitted this form of death, hastens to throw doubt on it! "It is probably doubtful whether these cases are due to ether-inhalation; as we shall see, the heart is practically uninfluenced for evil by ether." True, if there be added "*in animals*," but it is not true in man. Dr. Buxton says: "The action of ether upon the vasomotor system has been carefully worked out. Arterial pressure is always increased in ether-narcosis." But Kappeler says the reverse, and his observations were made upon man. In seven of ten sphygmographic tracings, "the curve of deep ether-narcosis differed not at all from that of chloroform-narcosis,"[1] and his conclusion from clinical study was that death in man under ether does not always begin with the respiratory process, and that ether-death does not differ essentially, in some cases, from chloroform-death. Ether-death by cardiac paralysis is distinctly recognized by Mr. Clover,[2] an authority which will certainly be accepted anywhere. This form of death is the exception, it is true, with ether, while it is frequent with chloroform.

---

[1] Transac. Amer. Med. Assoc., Vol. I.

[2] Journ. of Anat. and Phys., May, 1869.

[3] British Med. Journ., 1879.

[4] Klinische Vorträge, No. 80.

[5] Anæsthetica. Lief. xx. der Deutschen Chirurgie, Stuttgart, 1880.

[6] Richet, Accidents tardifs apres l'administration d'anæsthesiques, Gaz. des Hôpitaux, July, 1879.

There remains now to note some points not mentioned at all, or which do not receive the attention they deserve, even in papers not designed to be exhaustive. There is no observation or remark as to those fatal cases in which the dangerous symptoms make their appearance some considerable time after the administration of the anæsthetic has ceased. Such cases there are on record, after both ether and chloroform; and with our present knowledge they baffle all attempts at explanation, and offer an interesting and complex problem for solution.

There is not the slightest allusion here, in papers or debate, to the important fact, first recognized by Dr. Emmet, that a patient with albuminuria is a peculiarly bad subject for ether. The point is one of so great practical importance that it is singular not to find it in a canvass of the dangers of anæsthetics.

We looked with interest for, and think there was right to expect, something here as to the relative use of the two great anæsthetic agents, and as to their relative mortality in Great Britain. Some inference may be drawn, but there is no definite statement, and no statistics. Any one who reads a weekly British medical journal, cannot fail to be horrified by the frequently recurring announcement of a death from chloroform, nor to feel the deepest surprise that the profession there has not to a greater extent abandoned an agent which experiment and experience have both demonstrated to be far more dangerous than other anæsthetics at command. And still these papers and this debate show no marked disposition to change. The general tone is that ether is the safer anæsthetic. One would think that Dr. Buxton had made the dangers of chloroform pretty plain, yet in the very same paragraph in which he says that its use "is fraught with danger," he himself expresses the belief that it "is much maligned," which means, we suppose, that a good case has not been made out against it! Further, "as a practical anæsthetist myself, I should not hesitate in many conditions to administer chloroform, or a congener, rather than ether." The determining conditions he does not give. In the debate, we read that "all Scotch surgeons like chloroform, and it is the same among Scotchmen in London." Mr. Chiene is a Scotchman. After a death from chloroform in his wards he used ether for a year, and then returned to chloroform. Now he makes a bold plea for his agent. He says:

---

[1] Op. cit., pp. 158, 167.

[2] Quain's Dictionary, art. Anæsthetics.

> "The present outcry against chloroform is the result of an imperfect understanding of A, its physiological action; B, the proper method of administration; C, the dangers which may accompany its use, and their treatment; D, the dangers which follows its abuse, and their treatment."

If any one could transport himself back to the time Mr. Chiene's lecture was written, he might accept the professional feeling against chloroform as a mere "outcry;" with eyes open in the present, it is impossible not to see that our understanding of most of these points is far from imperfect. Prof. Fraser, in the debate, boldly stated his disbelief of "the difference alleged to exist in the relative mortality of chloroform and ether." And "nothing he had ever heard would lead him to believe that ether was distinctly preferable to chloroform." Prof. Fraser is an eminent teacher of materia medica, and lives in Edinburgh. With statistics now before the profession his position is astounding, but we can appreciate the point he makes that the use of ether, "only now commencing to extend" in Great Britain, the near future may show a different footing of the figures. It is precisely in the lack of statistics upon this point that the greatest disappointment is experienced in reading these papers. For some years ether has been gaining upon chloroform as an anæsthetic in that country, and it is there that a solution of the problem of the relative mortality of the two great anæsthetic agents is to be looked for. Here Kappeler looked for it. In his work published six years ago, he pays a handsome tribute to the public institutions of Great Britain, to the publicity of their administration, to the enterprise and conscientiousness of its medical press, calls attention to the vast field there presented for observations of anæsthetics, indicates that to that country we must look for a solution of this yet unsolved problem. The solution has not yet been furnished.

Finally, as to the relative danger of the two agents, ether and chloroform, we fail to find here a recognition of that one great point which should alone decide the question. That point is the irregularity of action of chloroform—the fact that it does not always act in the same subject in the same manner. To show that this is a fact we could quote abundant testimony, both practical and theoretical, but there is enough emanating from Great Britain alone. Clover recognized the irregularity of action of this anæsthetic and abandoned it. Anstie indicated it. Richardson says that chloroform shows "deviations of the usual order in which the various functions of the nervous system are affected." But nowhere is this point more prominently stated than in the report of the Anæsthetic Committee of the British Medical Association, and published in its own journal.[1] Chloroform acts so differently upon the same animal at different times, that this Committee is obliged to strain language and use a word which implies volition to express it—"chloroform has sometimes an unexpected and *capricious* effect on the action of the heart!"

---

[1] June 29, 1879.

It is this fact that has justly banished chloroform from general use as an anæsthetic; by this fact alone can it be explained that chloroform has caused death under all circumstances, in spite of the most minute precautions, and in the hands of the most skilful administrators, such as Snow, and Clover, and Kappeler. Yet this fact seems yet unrecognized by the profession of our sister country.

With hesitation to accept ether, and a recognition of the dangers of chloroform, we find no effort to escape the dilemma by resorting to mixtures. They receive very little mention. One speaker alone has lately used the A. C. E. mixture—one part alcohol, two parts chloroform, three parts ether, by measure. All he can say of it, is that he has found it "a very useful preparation." Another gravely asks what is the object of the mixture, and another says that all the ether will evaporate and leave only chloroform "*in a week!*" Doubtless he has been misreported. Mr. Baily, of London, is in the habit of giving four parts of ether to one of chloroform "in eye cases, and this did very well, too, in cases of ovariotomy and general surgery." But one surgeon has used mixed vapors systematically, and that has been by Clover's inhaler, giving first chloroform, and then turning on ether. Again we have to confess surprise. Mixtures were formally recommended by the Medico-Chirurgical Society of London as early as 1864; among the formulæ given was that of the A. C. E. mixture. Yet in the land of their birth they are scarcely recognized. It is impossible to frame an explanation. Mixtures are diluted chloroform. All experiments with animals have shown the importance of keeping down the percentage of chloroform vapor in the air, all inhalers have been designed to insure this, and we add that an immense clinical experience has been gathered in favor of simple dilution. There is the Vienna mixture, six parts of ether to two of chloroform, with more than 8000 administrations and no death; there is the weight of Billroth's authority in favor of three parts chloroform, one part ether, and one of alcohol; there is the valuable experience of Sir Spencer Wells, in favor of methylene, which is only diluted chloroform. Twenty years' experience with the A. C. E. mixture has given to the writer the firm assurance that this mixture is equally as safe as ether, while it is far more prompt in action and manageable during administration.

From these papers it appears that there has not been in Great Britain any considerable experience with, and consequently that there is no just appreciation of the value of, the combined influence of morphia and atropia, in modifying the action of anæsthetics, and in warding off their dangers. The practice of giving a preceding hypodermic injection of these narcotics is resorted to by few, its advantages are questioned by some, and but faint praise is given it by any. One speaker deems it "advisable" where shock is anticipated, or where the condition of the patient demands it—the particular condition not being stated. Another thought that "the administration of morphia undoubtedly possessed some advantages, as in avoiding shock; but the resulting narcosis was often very prolonged, and might cause alarm," which may be true, but depends entirely on the amount of the dose. Prof. Fraser "raised the question as to whether the administration of morphia and atropia beforehand was really of practical value." Mr. Chiene alone follows this plan regularly, and has done so ever since he resumed the use of chloroform. He, however, expresses no strong convictions in its favor. We trust that the history of this important modification of the anæsthetic process is better known in Europe than is indicated here. Mr. Chiene's "attention was directed to it by his house surgeon," and, "he learned it from Prof. Fraser's lecture." Surely Nussbaum and Bernard, who, in 1863, almost simultaneously discovered it, deserved mention, and especially the latter, who, in 1875,[1] gave to the world the experimental proof of the benefits to be derived from it. Just across the channel, theses and treatises have appeared upon the subject,[2] and ten years ago it was formally advocated in America.[3]

When we examine this mode of practice theoretically, we cannot but be astonished at the amount of favorable evidence which immediately presents itself. Anæsthetics produce death in various ways, the most frequent of which is by depression of the circulatory or respiratory powers; one or both of these fail more or less suddenly. Chloroform frequently causes death by a profound impression on the laryngeal and pulmonary, or on sensitive nerves, thus by reflex action, inhibiting the heart. Could we eliminate from the action of chloroform depression of the heart's action, and deleterious reflex influence, it would stand to-day without a rival as an anæsthetic. The only question remaining then is, whether we have any drugs which will stimulate and sustain cardiac and respiratory power and restrain reflex action. The question needs no answer. It is in every treatise on materia medica. The power of atropia as a stimulator of the two great vital functions has been emphasized by Harley, and Bartholow, and Fothergill, while the soothing and shielding influence of morphia against all irritations needs no mention. And now comes Lauder Brunton to add abundant testimony of the strongest character in support of this modification of the anæsthetic process.[4]

It is curious to see Dr. Buxton come right up to a recognition of the necessity of something of this kind and then pass on without mentioning it. He asks "if we may not so administer our chloroform as to obviate all possible risk of reflex syncope through stimulation of the vagal endings," and gives no answer to the question.

When it comes to clinical support, there is a great deal in favor of this mixed anæsthesia, besides that given in the authorities mentioned, and we know of none adverse. Personal experience with it has been extensive enough and varied enough to impress the strongest convictions. Nor is it in a single direction that benefit is derived. First, there is the emotional disturbance quieted, and there have undoubtedly been deaths purely emotional from dread of the anæsthetic, just as there were such deaths from dread of the operation before anæsthetics were known. Then there is the small quantity of anæsthetic required to produce and to maintain surgical anæsthesia. Again, there is the greater quietude of the patient from the more profound effect. Bernard could tie the lingual artery of the dog, deep in the throat, under the united effects of chloroform and narcotics, but could not do it with either alone. We have seen the method tried with the same anæsthetic (A. C. E.), by the same administrator, to the same patient, and for the same operation, and have been both surprised and pleased at the contrast. Over and over again we have seen patients go through ovariotomy without pulse or respiration wavering, and Richardson pointed out the marked reflex effect of thrusting a sponge into the peritoneal cavity. Over and over again we have seen them pass through this operation without moving a muscle or uttering a sound. We have seen them strapped down to the table in England. Then after the operation with this plan, hours are spent in quiet slumber and the after-sickness of the anæsthesia is lessened, because, as a general rule, it is in direct proportion to the quantity of the anæsthetic absorbed. These advantages are all independent of the question of danger, and that none of them have attracted sufficient attention to cause notice is passing strange. Stranger still, and a more striking proof of the entire want of appreciation of this method in Great Britain, is the fact that it is not mentioned in the paper of Dr. Talfourd Jones, read at the same meeting, on Hypodermic Medication.[1] Formal consideration is given in this paper to the antagonism of medicines, and full recognition of the powers of morphia and atropia as cardiac and respiratory stimulants, and yet their effect in counteracting the depressing effect of chloroform on the heart is not mentioned.

---

[1] Leçons sur les Anesthesiques, Paris.

[2] De l'anesthesie mixte, etc Par Verrlet-Litardiere, Paris, 1878. Recherches sur l'anesthesie chirurgicale obtenu par l'action combinée de la morphine et du chloroform. Par H. de Brinon, Paris, 1878.

[3] This Journal, April, 1876.

[4] Pharmacology, Phila., 1885.

"Belladonna and stramonium have a rather peculiar action, stimulating the respiratory centre, and at the same time appearing to lessen the excitability of the ends of the vagi in the lungs"—p. 224

"This drug [atropia] completely destroys the inhibitory action of the vagus on the heart"—p. 252.

"These two classes [among them is atropia] agree in destroying the inhibitory power of the vagus nerve."

In closing, we feel compelled to notice some points of doctrine to which we cannot give assent. They are all in Mr. Chiene's paper. This gentleman occupies a high position as a teacher, and enjoys an enviable reputation as a surgeon. Yet these facts but render the duty more imperative of combating doctrines which may mislead young practitioners and are fraught with danger to the patient. We will not oppose to any one of these mere theoretical disquisitions, but present the irrefragable evidence of clinical experience.

In his paper we read, in regard to the administration of chloroform, "No attention is to be paid to the pulse; it is the last thing that stops." In this he follows his predecessor, Lister, who did not hesitate to say that to consider the pulse the most important symptom "is a most serious mistake."[2] Now, in reply to this, it is not necessary to go beyond the first four cases of death, under chloroform, as given by Snow.[3] They were recognized as deaths by paralysis of the heart by Dr. Sibson, and commented on as such as early as 1848. "When the stoppage of the heart's action is due to the drug the patient is dead," says Mr. Chiene. This is true. But how about those cases seen over and over again where the heart's action failed and the patient was rescued? Dangerous symptoms, under anæsthetics, as well deserve study as cases of death. Of twenty-one cases of dangerous symptoms by Anstie, in sixteen a change in the pulse is most prominently noted, and, with pallor, was the first symptom observed.[4] In every work upon chloroform ever published, death beginning at the heart, has been made prominent under the various terms of "shock," "syncope," and "sideration," and clinical experience has furnished so many examples of this mode of death that it has been held by many to be typical of and peculiar to chloroform, and death by respiratory paralysis has been completely overshadowed. Yet we are told that it is a matter of no importance to watch the pulse!

Again, Mr. Chiene has some excellent remarks upon administration; that the administrator "must use his brains" is well put. But he says,

---

[1] British Med. Journ., Sept. 26, 1885.

[2] Holmes's Surgery.

[3] On Anæsthetics, London.

[4] Sansom, on Chloroform, pp. 87-89,

"it is a matter of no importance how much is poured on the towel, except as a matter of economy!" There is no more dangerous doctrine connected with the giving of chloroform than this. It is not necessary to go over again deaths from reflex action by powerful impressions on the laryngeal and bronchial nerves. Nor need we again refer to the report of the Grant Scientific Committee, and the observations of the sudden retardation of the heart's action upon every fresh application of chloroform to the sponge. But, clinically, what will Mr. Chiene do with Case II. of Snow's list? One such case is enough to settle the question; but of 133 fatal cases, 8 died immediately upon adding a new supply of chloroform to the sponge or apparatus![1] The clinical evidence is overwhelming.

We read further of "stoppage of the respiration and heart's action, directly due to a poisoning *by an overdose of the drug.*" Again Mr. Chiene is true to the doctrines of the Edinburgh school, and continues an error which, in the expressive language of Kappeler,[2] runs like a scarlet thread through British anæsthetic literature. "Chloroform is a cumulative poison," Mr. Chiene says. How has it been shown? Of the fatal cases, fifty per cent. have died before the stage of surgical anæsthesia was reached.[3] In other words, when a patient is "off," under chloroform, one-half whatever danger there may be in the process is passed. Again, of Snow's fifty fatal cases, nine died within a minute after commencing to inhale, and four of these within a few seconds. These immense clinical facts must be disposed of by any one who maintains that chloroform is "cumulative," or kills only by overdose.

But there is another important aspect of this doctrine, a direct corollary of the premise. It throws the blame of every fatal accident most unjustly upon the administrator. This is plainly taught by Lister: chloroform "is deadly when mismanaged; is free from danger if properly used."[4] The proposition is monstrous. Deaths from maladministration there have been; they are easily recognized; but those who accept and maintain the proposition that all deaths are from "cumulative" effect, or overdose, and consequently from want of skill, or care in administration, must also accept the proposition that Simpson and Snow, and Clover and others, of the best administrators the world ever saw, killed their patients by malpractice.

The query will doubtless arise, how it is that, with so gross a violation of what we believe to be an essential point in the administration of chloroform—its slow and gradual introduction—the mortality does not illustrate it? To this may be adduced the very imperfect extent to which the experience of any individual can form a basis for the calculation of mortality. The figures seem "capricious!" Thus, Mr. Clover had 5000 cases without accident, and then two deaths close together. One hospital in England had one death in about five hundred administrations. Nussbaum has given it forty thousand times without a fatal result. Second, we find herein an additional argument in favor of mixed anæsthesia. Since Mr. Chiene has resumed chloroform, he precedes the administration by a hypodermic injection of morphia and atropia. It is the influence of these narcotics in sustaining the respiration and circulation, in benumbing reflex sensibility, in harmonizing actions of the

[1] See Lallemand and Perrin, Cases 11, 12, 16, 18, 26, 35, 40, 47, 66.

[2] Op. cit.

[3] Sansom.

[4] Holmes's Surgery.

nervous system thrown into discord as anæsthesia is produced, which enables the patient to pass safely through even the dangers of maladministration. And we do not feel at all certain that if the more extended use of ether should show any considerable increase of mortality, that the anæsthetic of the future will not be chloroform with a preliminary injection of narcotics. J. C. R.

Micro-chemistry of Poisons, Including their Physiological, Pathological, and Legal Relations; with an Appendix on the Detection and Microscopic Discrimination of Blood. Adapted to the use of the Medical Jurist, Physician, and General Chemist. By Theodore G. Wormley, M.D., Ph.D., LL.D., Professor of Chemistry and Toxicology in the Medical Department of the University of Pennsylvania. Second edition. 8vo. pp. 784. Philadelphia: J. B. Lippincott Company, 1885.

The second edition of Professor Wormley's well-known treatise contains some valuable additions, but the scope and design remain unchanged. The most important of the additions, we are told in the preface, consist of illustrative cases, new tests and methods, and an appendix on the discrimination of blood. Numberless other improvements are, however, to be found in the text.

It may be said at once that the book is invaluable, and, indeed, absolutely essential to every toxicologist. It is no mere *rechauffè* of the labors of others, but is crowded with records of the personal work of the author, work which was much wanted, which required great patience as well as skill for its execution, and for which every scientific man must feel grateful. So great indeed is the value of the book, that the reviewer feels tempted to speak only of its merits and to ignore its defects. But it is necessary to say that it is very incomplete. It should have been smaller, or much larger, and in either case its practical utility would have been enhanced. The author might have confined himself to the subjects indicated by his title, and given us, with his admirable microscopic drawings, a complete account of the uses of the microscope in toxicology, and of limits of delicacy in chemical tests. Or, conferring a still greater boon, he might, as he evidently could, have made the book a complete manual of toxicology. As it is, the deficiencies are as obvious as its merits. Many poisons of great importance are omitted altogether, and many details with regard to other poisons which should appear in a complete treatise are absent. On the other hand, space might have been saved by the curtailment of purely chemical descriptions found in every text-book; as, for instance, in the pages devoted to the chemical properties of sulphuric acid and phosphorus. Among the omissions, we find poisons as important as the barium and chromium compounds, carbolic acid, nitro-benzene, chloral, colchicum, cantharides, ergot, savin, and croton, not to speak of the paraffins and terpines. In regard to several of these poisons, Professor Wormley's careful observations would have been very useful.

The introductory section of the book deals, as usual, with definitions, classifications, sources of evidence, general methods, and the like. It